

the termination of Nasmyth Inlet which agrees exactly with the description of its shape at page 280 of Mr. Knobel's Memoir; and certainly during that opposition there was no approach to the definite rounded form seen this year.

I am indebted to Mr. Denning of Bristol and Mr. Smart of London for several views of *Mars*; and the drawings of both these observers confirm the existence of Lassell Sea as a roundish spot.

One of the most marked features of the opposition now past has been the frequent appearance of masses of light on the limb which never arrive at the meridian, and that the orange portions of the surface so frequently seen on the meridian do not retain this colour as they pass towards the limb, but become whitish. May not this suggest that cloud condensation prevails on the right side of the planet, and that these masses of cloud are dispersed as they pass the meridian towards the Sun?

The character of the markings on *Mars*, the difficulty of observing, and still more the difficulty of drawing them, will continue to make this planet, if not a satisfactory, at least a most interesting, object of study.

*On the asserted Foreshortening of the Inner Side of the Penumbra of Spots when near the Sun's Limb.* By Rev. F. Howlett.

I am desirous of placing on record some definite statements and measurements in connection with the appearance of certain symmetrically shaped solar spots, both when near the centre and also when close to the limb of the Sun.

It will be found that these measurements entirely militate against the commonly received opinion that the spots are to any such extent sunk in the solar surface as to produce always those effects of perspective foreshortening of the inner side of the penumbra (when near the limb) which have been described in various works on Astronomy, and founded chiefly, I believe, on statements made as long ago as the year 1769 by Dr. Wilson of the University of Glasgow.

These statements and observations on the part of Dr. Wilson are so extremely circumstantial that I am far from desiring to call them in question, though I beg to be allowed to affirm that they at any rate do not always hold good.

Dr. Wilson's statements are thus alluded to in Mr. Proctor's very full and interesting popular work *The Sun*, ed. 1871, p. 169. "In November 1769 Dr. Wilson began the careful study of a large spot (visible to the naked eye). . . . When he first examined the spot (Nov. 22) it was situated not very far from the western or 'preceding' edge of the Sun's disk.

"On the next day he found that the spot had changed its appearance. The penumbra, which on the previous day was

equally broad on all sides of the nucleus [or as we should now say, more precisely, the umbra], was now very much contracted on the side which lay towards the centre of the disk, while the other parts retained nearly their former dimensions. On the 24th he again observed the spot. The distance from the limb was now only twenty-four seconds, and the contracted side of the penumbra had entirely vanished. . . . The breadth of the nucleus [umbra] on the same side also appeared to be more suddenly impaired than it ought to have been by the motion of the Sun (*sic*) across the disk.

“Dr. Wilson showed that these results correspond with those which would follow if the spot was a vast cavernous opening, having the nucleus at the bottom, and the penumbra forming its sloping sides.

“It only remained to be seen whether a corresponding succession of changes occurred when the spot reappeared on the eastern edge, and thence passed across the solar disk. This actually happened. On December 11 the spot appeared on the opposite side of the disk. The side of the penumbra next the edge, which formerly vanished, was now wholly visible, while that turned towards the centre of the disk appeared to be wanting. On December 12 it [that is, this part of the penumbra] came into view, and he saw it distinctly, though narrower than the other side. He did not see the spot again till December 17, when it had passed the centre of the disk, and the penumbra appeared to surround the nucleus [umbra] equally on all sides.”

Not, however, that such statements, however well attested on the above occasion, have always remained unchallenged. For instance, on the occasion of the eleventh General Assembly of the Astronomical Society, convened at Geneva in the month of August 1885, Professor Spörer of Munich, in a copious *résumé* of his labours in connection with the solar spots, denies that the spots possess the character of funnels (*tonnoirs*), which is attributed to them in the greater number of works on elementary astronomy. The appearances which attend the disappearance of spots on the Sun’s western limb cannot, he affirms, be explained by such a theory. “When a spot,” he says, “is very shortly about to disappear, by virtue of the Sun’s rotation on his axis, the two *lateral* borders of the penumbra vanish from sight, whilst the nucleus [umbra] remains visible, together with the fragments of the surroundings (*pourtour*) to the north and south; the whole phenomena,” he says, “presenting the appearance of a veiled brightness (*éclat voilé*) to such a degree that one might easily confound the nucleus [umbra] itself with its surroundings,” and of which typical and striking instances may be seen in vol. ii. sheet 99, and still more sheet 108, of my drawings.

Spörer then seeks to explain this appearance by a heated facular border surrounding the spot, and causing an ascending current of extremely heated gas, through which it is with the

greater difficulty that the spot is discerned the nearer it is to the limb.

He then remarks, very justly, that it is "by no means the case that all spots are suitable for the estimation of the pretended parallax of the depth of a spot; and in making use of documents on this subject one is far from arriving at results similar to some of those which have been published; and that in all attempts to make drawings of the spots there is a danger of falling into grave illusions."

In my contention, again, that by no means the majority of spots present that funnel-like appearance attributed to them by the Wilson hypothesis, I am sufficiently borne out both by Father Perry and Mr. Ranyard. The former, after paying especial attention, at my request, to this point in connection with a large and unusually symmetrical spot which was close to the western limb on the afternoon of April 12 of this present year, wrote me thus:—"The question of the foreshortening was specially attended to, and the observation showed a very slight excess of foreshortening on the side of the penumbra nearest the Sun's centre, but certainly not enough to give any support to Wilson's theory." Mr. Ranyard, again, in a letter of the 7th inst., reports:—"I had a good look at your spot on the morning of Wednesday, June 2, and I again had a search for it on the morning of June 4" (when, I may observe, it had quite passed off the limb). "I quite agree with you," he says, "that there was no appreciable difference in the breadth of the penumbra on the preceding and following sides of the nucleus" (umbra, again, is here meant) "when I observed it on the morning of Wednesday, June 2. I was observing it between 9<sup>h</sup> 20<sup>m</sup> and 10<sup>h</sup> A.M., and I thought that the penumbra was a little *darker* on the preceding than on the following side, but not very noticeably so. There was no band of increased darkness such as would have been produced by a column of absorbing matter rising vertically above the nucleus. I quite agree with you that the phenomena observed did not tend to support Wilson's hypothesis."

With respect to this last spot, Father Perry's observations do not confirm my own to the same extent, apparently, as was the case in connection with the spot  $\gamma$  of April last, though it is on that of June 2 that I would rely, if possible, more firmly than on the rest. Together with some small but beautifully executed copies of the sketches of spots made recently at Stonyhurst, Father Perry wrote as follows:—"You will notice that near the edge there appears to be a slight confirmation of Wilson's theory at both limbs of the Sun." And he adds, "In some of our drawings of solar spots this is much more marked; but these probably are different classes of spots, and all certainly do not behave alike."

But now as to actual measurements of what I would term the crucial spots  $\gamma$  of April 12, and  $\lambda$  of June 2.

I first noticed  $\gamma$  on April 2, at 7<sup>h</sup> 30<sup>m</sup> A.M., when it was about

$2' 48''$  from the eastern limb, and perceiving that the umbra was nicely central, and the whole spot neatly oval, I resolved to keep the spot under observation, with the view of testing the hypothesis in question. It was then about  $50''$  in length from north to south, and, as then, foreshortened on the sphere, about  $40''$  in breadth from east to west. On April 6 the size of the spot was somewhat diminished, but its neatness and suitability for testing the hypothesis was still more apparent, for a more perfectly central umbra or symmetrical penumbra could not be wished for. The spot at  $10^{\text{h}} 20^{\text{m}}$  A.M. that day was not far from the centre of the disk, and subtended about  $45''$  in length and the same in breadth. When at length, on April 12, the spot had arrived very near to the western limb, I made the following notes: At  $4^{\text{h}} 20^{\text{m}}$   $\gamma$  was only about  $10''$  from the western edge of the Sun, and subtended not more than  $10''$  in breadth. The umbra still appeared quite central, with a width of not more than  $2''$ . Each side of the penumbra measured also about  $2''$  in breadth, or possibly the "preceding," or outer, side,  $2''$ , and the following  $1''8$ , but of this I could not be quite certain.

I may observe also, in passing, that on May 9 a spot lettered  $\iota$ , when about  $18''$  only from the western limb, and measuring  $40''$  in length and  $8''$  in breadth, presented a perfectly central umbra  $3''$  in width; here again militating against Dr. Wilson's hypothesis.

But now for the spot  $\lambda$  of June 2, 1886. This was also a remarkably symmetrical one, of a large size, and specially adapted to the investigation in hand. On June 1 the spot was, at  $5^{\text{h}} 30^{\text{m}}$  P.M., just one minute from the western limb, yet there was not the slightest appearance of any foreshortening of the *inner* side of the penumbra. In fact, if there was a difference, the advantage was by about  $1''$  or so on the *inner* side. The spot now subtended about  $45''$  in length, by about  $17''$  in breadth.

On June 2 (the day on which I had invited Mr. Ranyard, and also again Mr. Perry, to specially watch for Wilson's phenomenon) I made these notes: At  $7^{\text{h}} 50^{\text{m}}$  A.M. the outer border of the penumbra of the spot  $\lambda$  was just  $20''$  from the western limb. The whole spot subtended  $45''$  in length of heliographical latitude, but so foreshortened in directions of longitude as to subtend not more than  $10''$  in width.

The *umbra* was as nearly as possible central, and having an apparent width of  $3''$ . Both the preceding and following sides of the penumbra were of the same width, viz.  $3''$ , though if there were a difference, the "following," or *inner*, side was the wider of the two, by perhaps  $0''5$ . There was at least a sensible difference (though exceedingly small, truly) in the opinion both of myself and a perfectly impartial fellow-observer, who, in fact, did not know what was expected one way or the other.

I regret that there should have been any divergence of opinion between Father Perry on the one side and Mr. Ranyard and myself on the other, though that divergence is evidently

extremely slight. What we require, however, in such investigations are actual measurements by micrometrical appliances.

Since writing the above I have carefully looked over the four last volumes of my drawings of solar spots, deposited in the library of our Society, extending over a period of ten years, from 1864 to 1873 inclusive, and find that during that period, whilst there are eleven instances fairly adverse to any phenomena of the foreshortening in question, there are seven instances fairly in its favour, but generally only to a slight degree.

With regard, however, to the very marked divergence between Dr. Wilson's observations of the last century and those of both Mr. Ranyard, Father Perry, and myself on this present occasion, we shall probably come to the conclusion that there are different classes of spots, and which, amongst other peculiarities, manifest very different degrees of profundity, as well as of general form, permanency, and even colour, as was observed in the month of April last by Mr. B. J. Hopkins, as well as on other occasions by Mr. Lockyer, myself, and, I doubt not, other observers.

Lastly, it may be as well to remember that the Sun's vastly deep and diaphanous atmosphere (whose existence is evinced by the very considerable diminution of the Sun's luminosity all round the margin of that luminary) must have some effect in modifying the appearances of all features situated on and near the limb. The effect of the refraction of the rays proceeding from the Sun's surface would be to cause somewhat more than half the disc to be visible to an observer on the Earth, and hence to retain solar features in sight which would otherwise have passed out of view; and also to flatten and level prominences standing on the limb, and possibly also to slightly increase the apparent width of the outer side of a penumbra somewhat more in proportion than the inner side, but from a different cause to that assigned to this by Dr. Wilson.

It was Professor Challis, I think, who many years ago drew attention to this modifying of objects on the Sun's limb, by virtue of the refraction exerted by the solar atmosphere.

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*The Fourth Satellite of Jupiter during Superior Conjunction on the Night of April 5, 1886. By Edmund J. Spitta.*

A frequent explanation of the anomalous appearance which the satellites of *Jupiter* sometimes present during transit, but which is scarcely admitted to be adequate or satisfactory, is based upon the suggestion that the illumination of the hemisphere of the satellite then presented to the Earth is not equally distributed; and this explanation has been still further complicated by the suggestion that owing to the period of rotation possibly corresponding with that of revolution, this particular hemisphere is only seen at the time of transit, and that then the effect of the unequal illumination is aggravated by the contrast with the bright disk of *Jupiter*.

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